

a body acted upon by two forces whose direction and intensity may be conveniently represented by the adjacent sides of a parallelogram. The body *really* moves along the diagonal; *virtually* it has a double track, one along each of the adjacent sides of the parallelogram. In like manner, in our hydrocarbon-oxygen system, we may picture two compelling forces, viz. the tendency of oxygen to combine with carbon, and of oxygen to unite with hydrogen. You may, if you please (and this seems a fascinating exercise to Dr. Arms'rong), shut your eyes alternately to each force and say, "first the hydrogen gets all the oxygen, and then the carbon snatches some from it," or you may just as well put it the other way about. I simply recorded the fact that the carbon got the most of the oxygen, with an explicit reference to the fact that I was dealing with the cooled products. Before my experiments were published Dr. Armstrong thought that hydrogen got most of the oxygen. He had actually tried to persuade Sir G. G. Stokes and many others to this effect. When my paper appeared he sought to discount the facts it contained by flights of polysyllables worthy of a great statesman.¹ Is it not strange that he should now turn to rend the man who relieved him from what he so abhors—a dogma?

The assumption that the products which are collected from a flame may have totally altered in kind during a minute fraction of a second is perfectly gratuitous, and a similar assumption might be made about nearly every reaction in chemistry. Dr. Armstrong might just as well forbid me to say that zinc and hot sulphuric acid give sulphur dioxide, as to say that when a hydrocarbon burns with limited oxygen, the carbon has the preference.

I will not trespass on your space with a discussion of the liberation of carbon in luminous flames. Dr. Armstrong's contentions on that matter are of precisely the same character as those I have dealt with above. Dr. Frankland has promised us some new evidence in favour of his theory. I trust I have always treated this theory with respect. I am not bigoted on the subject, though I await Dr. Frankland's promised publication with the same sort of feelings as those with which a Neapolitan might look forward to the reawakening of Vesuvius.

I have now, I hope, given an adequate reply to the question of scientific fact raised by Dr. Armstrong. I will not say much about the imputations he casts upon my scientific honesty. It ought not to be a light thing for a man in Dr. Armstrong's position to accuse a scientific worker of deluding an audience into unsound opinions by means of dazzling experiments, of playing to the gallery; of doing, in short, just those things which are most repugnant to the conscience of an earnest investigator. After years of personal friendship, I know Dr. Armstrong's idiosyncrasies very well, and they are, I imagine, pretty well known to the scientific world in general. I feel compelled, none the less, to ask him either to justify or withdraw his aspersions. I make no appeal *ad misericordiam*, and seek no comforting eulogy. It is a duty Dr. Armstrong owes no less to the scientific world than to myself to state clearly and precisely how I have departed from the standards of diffidence, deliberation, and exactitude that are becoming to a man who is honestly seeking to expound the truth. This, at any rate, is a matter the settlement of which is not contingent upon the arrival of that chemical millennium when we shall recognise "chemical interchange and electrolysis as interchangeable equivalent terms"; and I have the right to ask for an immediate and definite reply to my demand.

ARTHUR SMITHELLS.

December 2.

The Second Law of Thermodynamics.

CLAUSIUS' supposed deduction of the second law from the ordinary equations of dynamics in the form

$$\frac{\partial Q}{T} = 2\partial \log(iT)$$

has been discussed at length by Messrs. Larmor and Bryan in

¹ Here is a quotation from one of his letters to Sir G. G. Stokes: "Regarding the interactions in flames as consisting in a series of simultaneous and consecutive explosions, of which we can only examine the final steady state, it seems to me that the phenomena are necessarily of an excessively complex character, and that their appreciation and successful interpretation must tax our powers of mental analysis in a high degree. It will certainly be unwise at present to infer that the oxidation of the hydrocarbons, or the separation of carbon and also of hydrogen from them, takes place entirely in any one way." This seems to me like saying of a fall downstairs, that it is "a series of simultaneous and consecutive" bumps, &c. so difficult to trace out and presenting so many possible varieties of motion that it is hardly safe to call it a fall down stairs at all.

their Report on Thermodynamics for the British Association. They accept the deduction on condition that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential.

Now it is admitted that the equation

$$\frac{\partial Q}{T} = 2\partial \log(iT)$$

can be proved for a conservative system with the meaning of i given in the report. But in order that this equation, however true, may express the second law, T and i must be *independent variables*, or (which is the same thing when there is only one controllable coordinate v) T and v must be *independent variables*.

Now the second law implies comparison of two states, in either of which a substance can exist permanently. So if we seek to prove the law, or an analogous law, for a dynamical system, it is essential that we should compare two states of the system in either of which it is in *stationary motion*. One state may have the variables T and v , and the other may have $T + \partial T$ and $v + \partial v$, but there must be *stationary motion* with either pair of values. If then K be the virial of all the forces, external as well as internal, the Clausian equation, $K = 2T$ must hold. But if the system be conservative, as Larmor and Bryan assume, K is a determinate function of v , and the virial equation constitutes a relation between T and v , so that only one of them is independent. For example, a fixed quantity of gas in equilibrium in a vertical cylinder under a piston of mass " m " acted on by gravity. Clearly if T be given, v , the volume, is determinate, or we have only one independent variable. If m be disposable, you may make T and v vary independently, but then the system is not conservative. It seems to me that Larmor and Bryan's equation does not express the second Law.

Prof. J. J. Thomson, in his "Application of Dynamics to Physics and Chemistry," pages 95-100, proves the second law on a certain assumption. And Boltzmann, "Über die Mechanischen Analogien des zweiten Hauptsatzes," has proved it on, I think, the same assumption. In order to show clearly the nature of the assumption I will begin a proof as follows, treating only the case of T temperature, and v volume. If χ denote the potential of all the conservative forces, p the external force necessary to maintain v constant, we have T being the mean kinetic energy of one of N molecules

$$\partial Q = N\partial T + \bar{\partial \chi} + p\partial v.$$

But by the virial equation

$$p\partial v = \frac{2}{3}NT\frac{\partial v}{v} - \frac{\bar{\partial \chi}}{\partial v}\partial v,$$

in which $\frac{\bar{\partial \chi}}{\partial v}$ is to be distinguished from $\frac{d}{dv}\bar{\chi}$ as explained in Watson's "Kinetic Theory of Gases."

Therefore

$$\partial Q = N\partial T + \frac{2}{3}NT\partial \log v + \bar{\partial \chi} - \frac{\bar{\partial \chi}}{\partial v}\partial v,$$

or

$$\frac{\partial Q}{T} = N\partial \log T + \frac{2}{3}N\partial \log v + \frac{1}{T}\left(\bar{\partial \chi} - \frac{\bar{\partial \chi}}{\partial v}\partial v\right),$$

now make

$$i = \frac{v^{\frac{1}{2}}}{T^{\frac{1}{2}}}$$

(a definite time), then

$$\partial \log T + \frac{2}{3}\partial \log v = 2\partial \log(iT),$$

and therefore

$$\frac{\partial Q}{T} = 2N\partial \log(iT) + \frac{1}{T}\left(\bar{\partial \chi} - \frac{\bar{\partial \chi}}{\partial v}\partial v\right).$$

Now J. J. Thomson assumes (p. 97) that $\bar{\chi}$, in his notation V , is to be a function of v only, whence it follows that

$$\bar{\partial \chi} - \frac{\bar{\partial \chi}}{\partial v}\partial v = 0,$$

as he says, and so

$$\frac{\partial Q}{T} = 2N\partial \log(iT),$$

which I submit as a form of his result (118). I understand Boltzmann in the treatise above cited to make the same

assumption. J. J. Thomson subsequently considers the case of χ being a function of T as well as of v (p. 100). But he does not in this case make $\frac{\partial Q}{T}$ a complete differential.

I think that in the general case we must regard χ as a function of the unconstrainable coordinates, and as varying from one configuration to another, through which the system passes in the same stationary motion with constant v . When v becomes $v + \partial v$ we do work in two ways. Firstly, we alter the value of χ for each configuration, doing thereby on the whole an amount of work equal to $\frac{d\chi}{dv} \partial v$. Secondly, we alter the comparative frequency of different values of χ in the stationary motion. This is essential; for without this the system would not be in stationary motion with the altered values of T and v . I think J. J. Thomson had this in his mind when he made χ a function of T as well as v (p. 100).

Let, then, $f dx_1 \dots dx_n$ or $f d\sigma$ denote the frequency of the configuration $x_1 \dots x_n$, so that

$$\bar{\chi} = \int f \chi d\sigma, \quad \frac{d\bar{\chi}}{dv} = \int f \frac{d\chi}{dv} d\sigma,$$

and

$$\partial \bar{\chi} = \int f \partial \chi d\sigma + \int \chi \partial (f d\sigma)$$

$\partial f d\sigma$ referring to variation of the limits of integration

$$= \int \chi \partial (f d\sigma) + \frac{d\bar{\chi}}{dv} \partial v.$$

and so

$$\partial \bar{\chi} - \frac{d\bar{\chi}}{dv} \partial v = \int \chi \partial (f d\sigma)$$

and

$$\frac{\partial Q}{T} = 2N \partial \log (i T) + \frac{1}{T} \int \chi \partial (f d\sigma).$$

Now how to make

$$\frac{1}{T} \int \chi \partial (f d\sigma),$$

or

$$\int \frac{\chi}{T} \partial (f d\sigma),$$

a complete differential?

If

$$B = \int f (\log f - 1) d\sigma$$

(Boltzmann's minimum function),

$$\int \log f \partial (f d\sigma) = \partial B + N \partial \log v,$$

and is a complete differential.

Hence one solution, and probably the only general solution, of the problem is obtained by making $\log C f$ proportional to $\frac{\chi}{T}$, or

$$f = C e^{-\frac{c\chi}{T}},$$

where c is numerical. That gives

$$\frac{\partial Q}{T} = 2N \partial \log (i T) - \frac{1}{c} \partial B - \frac{N}{c} \partial \log v.$$

Since $2iT$ is the Action of the system during the definite time i , we see that the second law stands in a certain relation to the principle of least Action. But I think the complete treatment of it must be based on the virial equation. And it may be regarded as the law of the variation of B when T and the controllable coordinates vary.

S. H. BURBURY.

THE LOSS OF H.M.S. "VICTORIA".¹

III.

LAST week we discussed the opinions expressed by the Board of Admiralty, in their Minute of the 30th of October, upon certain points that relate to the construc-

¹ Continued from p. 127.

tion and stability of the *Victoria*; but the remainder was left for consideration in the present concluding article.

The value of an armour-belt at the ends for resisting damage.—Their lordships say "the fact that the *Victoria* was not armour-belted to the bow had no influence upon the final result of the collision. No armour-belt could have prevented the ripping open of the bottom below water by the ram-bow of the *Camperdown*, and the flooding of the compartments to which water could find access through the breach." Mr. White argues strongly against the assertion, which he states has been made, that if a strong armour-belt had existed at the place where the blow was struck, the damage might have been greatly reduced and the ship kept afloat. He considers that all the most important compartments which were flooded in the *Victoria* must have been thrown open to the sea under the conditions of the collision, even if there had been such a belt. "The breach in the side might have been different in form and possibly less extensive, especially above water; but it must in any case have been of large extent, and have admitted very large quantities of water in a short time." Mr. White proceeds to argue that the extent to which the *Camperdown* penetrated into the interior of the *Victoria* was not altogether a disadvantage, as the *Camperdown's* bow thus became virtually locked in the protective deck of the *Victoria*, till the relative forward movement of the latter ship was destroyed and the tearing action of her spur upon the side of the *Victoria* was thereby prevented. "Under the assumed condition of a non-penetrable armour-belt, this relative forward movement and tearing action must have taken place." But the Admiralty cannot admit the assumption of impenetrability. Reference is made to cases of collision, such as those between *Vanguard* and *Iron Duke*, and between *Grosser Kurfürst* and *König Wilhelm*, which prove, in Mr. White's opinion, that "the existence of an armour-belt is no sufficient safeguard against injuries resulting from serious collision."

The objections that have been made to leaving so much of the ends of some of our first-class battleships unprotected by armour, have been mainly in connection with their defence against gun-fire. The gun is, and appears likely for some time to be, the weapon of attack which a battleship must be designed primarily to resist. The attack of the ram can often be evaded by speed or skilful handling; and that of the torpedo by watchfulness, tactical resource, and smart conduct on the part of the officers in command. The real defence against rams and torpedoes lies at present much more in the judgment and skill with which a ship can be safeguarded or manœuvred by her officers, than in her own intrinsic power of resistance.

At the same time, it is obviously desirable that everything possible should be done to increase the amount of resistance that can be offered by a ship's hull to attack from ram or torpedo. The Admiralty say that an armour-belt would have no influence upon the final result of ramming. This statement is based upon two assumptions: (1) that "under a blow of such energy as was delivered on the *Victoria* the strongest armoured side ever constructed must have yielded and been driven in. Its water-tightness and that of the bulkheads, &c., within it adjacent to the place where the blow was struck, must have been destroyed, and the ultimate result (as regards the admission of water) would have been practically as serious under the same conditions of open water-tight doors, &c. as that which actually occurred in the *Victoria*"; and (2) that if the *Camperdown's* bow had been prevented by an armour-belt from penetrating to so great a depth as was stated into the side of the *Victoria*, her spur would have torn away much more of the bottom plating than it actually did.

The truth of both these assumptions appears very